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Development of Mathematical and Theoretical Physics in the 19th and 20th Century

Scholars of the 18th century such as Leonhard Euler, Daniel Bernoulli and Johann Heinrich Lambert had been mathematicians as well as physicists. With the accumulation of knowledge, the development of specific methods and the increasing number of scholars the scientific community splitted up. Mathematics and physics developed to independent disciplines with specific professorships, journals and of course specific methods. Mathematical physics, or theoretical physics, as it was called later on, was widely neglected or even despised by physicists and, for different reasons, as well by mathematicians.

There had been always important scholars working in the field, but nevertheless this negative attitude had an impact on the development of theoretical physics. In 1989 Wilhelm Wien described the situation:

> Theoretical physics in Germany nearly lies fallow. The reasons are firstly that physicists are working nearly exclusively experimentally with no interest for theory, secondly that most mathematicians have turned towards the most abstract topics and don't care about applications. The situation is illustrated by the fact, that pure theoretical physics is represented by two chairs only (Berlin and Göttingen) and that such an important chair as Munich had been cancelled. Nobody is now interested in theoretical physics. This will change later on, because otherwise physics as a whole would collapse (1).

As we now know, this vision about the rise of theoretical physics was confirmed by the events. But first we have to speak about the 19th century, to which most of this paper is devoted.

As we will see, society as a whole played a rather important role. In taking social conditions into account, I will go beyond the pure intellectual developments, on which today's thematic session will concentrate. Speaking about social conditions, it makes sense to concentrate on one country. I have chosen Germany, for the obvious reason that the archives are best available for me, but also for the fact that in this country the rise and fall of theoretical physics was

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(1) Letter of Wilhelm Wien to Arnold Sommerfeld, 11 Jun 1898, Sommerfeld Collection, Deutsches Museum Munchen.

more extreme than in any other case. I will speak first about the scientific community of physicists and then about the mathematicians to demonstrate how prejudices developed against mathematical physics in the 19th century. After that I will deal with mathematical physics for itself.

1. The 19th century

1.1 – *The physicists*

For the physicists Isaac Newton was the great hero. They repeated his "*hypotheses non fingo*" in all versions and expressed an extremely negative attitude towards speculation. "No step without experience and without experiments"(2), Georg Christoph Lichtenberg of Göttingen advised all his collegues at the end of the 18th century.

The Heidelberg physicist Georg Wilhelm Muncke argued in 1833, that physics "needs much more observations and experiments than calculus and geometry" (3). Johann Christian Poggendorff, the editor of the *Annalen der Physik und Chemie*, explained in 1847, that his journal had to deal with experimental researches, and that he had to refuse theoretical articles. He indeed refused in 1842 and in 1847 the papers of Julius Robert Mayer and of Hermann von Helmholtz on the conservation of energy. — Gustav Magnus of Berlin gave repeatedly serious warnings to young physicists not to deal too much with mathematics (4).

Only experimental physics was regarded real professional physics. It was extremely important therefore for a physicist, to have, in his collection, good instruments. The apparatus required for research and demonstration in the lectures became more and more expensive, and everywhere the physicists applied for funds. The governments of the various kingdoms and grand-duchies really gave money. Such a collection of instruments, generally stored in a special room, was called *cabinet de physique*. Around 1830 the physicists were not longer happy with their cabinets and wanted institutes: "The physical institute is nothing else than the workshop of the physicist", Franz Neumann of Königsberg explained in 1846 to his government (5).

In the revolution of 1848 as other groups and classes the physicists had their hopes. Above all they wanted for every Prussian and German university an own institute of physics (6). Such an institute was built actually at every university

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(2) *Georg Christoph Lichtenbergs physikalische und mathematische Schriften*, Bd. 4, gesammelt und herausgegeben von L. Ch. Lichtenberg und F. Kries, Gottingen 1806, 391.

(3) J. S. T. Gehler, *Physikalisches Wörterbuch, neu bearbeitet ...*, Leipzig 1825-1845, vol. 7/1, 1833, 510.

(4) L. Koenigsberger, *Herman von Helmholtz*, 3 vols., Braunschweig 1902-1903, vol. 1, 69.

(5) F. Neumann, *Erinnerungsblätter von seiner Tochter Luise Neumann*, Tubingen 1907, 373.

(6) Anonymus (= Gustav Karsten), *Von der Stellung der Naturwissenschaften, besonders der physikalischen, an unseren Universitäten*, Kiel 1849.

and polytechnical highschool, with a delay of two decades however. After the Franco-Prussian war there was a boom of new institutes. Of interest for us is the social structure at these institutes. Similar to the society of the time there was a clear hierarchy in the academic world. The university professors were divided into three groups. The lowest grade was the *Privatdozent* with (generally) no payment at all; second the associate professor, a permanent position but nearly without any academic rights, and at the top the full professor or ordinarius. Only the ordinarius was member of the faculty, with the power to decide in matters of his field.

This hierarchy was established as well at the institutes. The full professor was nominated director of the institute, and he alone decided whom he was willing to give permission to work with the instruments. It was *not* a rare exception, but rather regular, that the institutes were off limits for junior physicists, off limits even if they were associate professors. One of many examples is Ferdinand Braun, who got later on the Nobel prize together with Guglielmo Marconi. As he explained 1879 in a letter to the ministry, he was, according to his scientific convictions, depending on experimental researches. But he could not get permission from the chair-holder to use the state owned instruments (7).

There were only two possibilities: to find another position (which was extremely difficult) or to work on theoretical physics. So it was quite regular (and indeed part of the nomination contract), that the associate professor was responsible for theoretical physics (8). So, theoretical physics was a sort of an embarrassment. It was the second choice, the off limits for experiments. The field was definitely less attractive also from another point of view, the financial point. Part of the salary of a professor came from the students, who attended the courses. In ordinary or experimental physics students came from physics, chemistry and medicine; in theoretical physics only advanced students of physics attended the courses, and so the numbers were much lower here.

At each German university there was in the position of an associate professor or *Privatdozent* one, or more physicists, lecturing on theoretical physics and waiting for a full professorship to be able to do experimental physics. These rather high numbers of theoretical physicists had one advantage for the field. Until about 1870 lectures in mathematical physics had been held by physicists as well as by mathematicians. Towards the end of the century theoretical physics was regarded as part of physics. In the official Prussian regulations for secondary school teachers theoretical physics was comprised in physics and not in mathematics (9).

I now want to show why theoretical physics came into the hands of the physicists.

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(7) F. Kurylo, *Ferdinand Braun, Leben und Wirken...*, Munchen 1965, 77; M. Weber, *Wissenschaft als Beruf. Vortrag, gehalten im Winter* 1919..., München und Leipzig 1930, 570.

(8) Ph. Lenard, *Erinnerungen eines Naturforschers*, Heidelberg 1943, 97 (unpublished).

(9) W. Lorey, *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des* 19. *Jahrhunderts*, Leipzig und Berlin 1916, 260.

1.2 – *The mathematicians*

The reputation of theoretical physics, which was low in the community of physicists, was even lower amongst mathematicians. Why? German universities were formed according to the principles of Wilhelm von Humboldt. The old *l'art pour l'art* was one of these principles: Science must be done for itself, and not to achieve benefits. In mathematics purity became a strong postulate by Karl Weierstrass of Berlin and his influencial mathematical school. Important discoveries were made in pure mathematics throughout the 19th century, but the impact on the applications was rather restricted (10).

Where from do we have the professors of mathematics at our technical highschools? — the director of one of these schools asked, and gave the answer for himself. Generally our lectures of mathematics had worked in their younger years with a famous professor at one university [Weierstrass at Berlin]. They have studied carefully advanced topics in the theory of numbers and higher elliptic functions, but gained no experience at all in what we need, géometrie déscriptive (11).

Carl Runge explained in 1903, that the vast majority of mathematicians are going into the wrong direction. According to Runge these mathematicians were all working on artificial problems without contact to reality and without any sense for the art of calculating. Runge described the typical representative of this majority:

It never happens to him to solve a problem leading to a differential equation until the very end, I mean until the numerical evaluation with definite constants. Everyone must admit however that one does not fully master a theory if he is unable to apply it to the special case (12).

Felix Klein, born in 1849 and 35 years younger than Weierstrass, was a strong opponent of the Weierstrass' conception. Klein made himself a great name in mathematics, but decided later to devote most of his time for science policy. He stimulated cooperation between mathematicians, physicists, engineers and industrial managers, worked for the reformation of teaching and started the *Mathematical Encyclopedia*. Klein cooperated closely with the Prussian ministry of education and with industry. He founded the Göttingen Association for the Advancemente of Applied Mathematics to stimulate cooperation between science and industry for the benefit of both sides, and to collect money for science. With money from industry five new institutes in Gottingen were created, for example an institute for applied mathematics. Carl Runge was appointed full professor. He developed numerical methods to solve differential equations, which were used in these days but of course much more today.

(10) F. Klein, " Über die Beziehungen der neueren Mathematik zu den Anwendungen ", *Zeitschrift für den mathem. und naturw. Unterricht*, *26*, 1895, 535-540.
(11) G. Holzmuller, " Über die Beziehung des mathem. Unterrichts zum Ingenieur-Wesen... ", *Zeitschrift fur den mathem. und naturw. Unterricht*, *27*, 1896, 468-480.
(12) I. Runge, *Carl Runge und sein wissenschaftliches Werk*, Göttingen 1949, 115.

Felix Klein convinced the young mathematician Arnold Sommerfeld of his ideas. In 1898 Sommerfeld was nominated full professor at the technical highschool of Aachen. The greatest success was, that this professorship was not for mathematics, but for technical mechanics. Sommerfeld wished to demonstrate to the professors of the engineering departments, that it is possible to develop technical mechanics on a mathematical basis both for research and for teaching. Sommerfeld solved with mathematical methods some classical problems of engineering, and his collegues at Aachen were quite impressed. When after some years Sommerfeld left the place this was regretted by the engineers, but they did not take a mathematician as his successor. The general feeling was that Sommerfeld is a mathematician of a special kind, different from nearly all the others. When in 1920 the technical highschool of Stuttgart had an opening for a professorship in electrotechnology, they explicitly tried to find another " Sommerfeld ".

To summarize in short: Klein and Sommerfeld convinced quite a number of engineers, that in the various fields of technology mathematical methods could be used successfully. Amonst mathematicians however, the majority remained quite hostile against Felix Klein and his science policy. When Sommerfeld published his theory of lubricant friction, the mathematician Edmund Landau called all applied mathematics " lubricant oil " as something, with which the real and pure mathematicians does not want to make himself dirty (13). In 1906 Sommerfeld was appointed full professor for theoretical physics at the university of Munich. His promoter had been the physicist Wilhelm Conrad Röntgen. There was only one strong opponent: the mathematician Ferdinand Lindemann. A representative of " pure mathematics ", Lindemann had been the first academic teacher of Sommerfeld, before Sommerfeld decided to support Klein's position. Lindemann was furious. Sommerfeld's papers, he wrote in his counterproposal, are objectionable from the mathematical point of view (14).

1.3. – *Mathematical physics (1800–1850)*

After having discussed the attitude of physicists and of mathematicians in general lines, I now will discuss the development of mathematical physics for itself. This will bring us back, inevitably, to the opinion of the physicists.

In 1822 Joseph Fourier published his *Théorie analytique de la chaleur*, which was called later on the " bible of mathematical physics ". Heat is regarded as a special form of matter without weight. It passes through a conductor, for example a cylinder of metal. Mathematically the problem is to solve a differen-

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(13) A. Hermann, « Sommerfeld und die Technik », *Technikgeschichte*, *34*, 1967, 311-322; *Weltreich der Physik*, Esslingen und Munchen 1980.

(14) U. Benz, *Arnold Sommerfeld. Lehrer und Forscher...*, Stuttgart 1975; M. Eckert, W. Pricha, et al., *Geheimrat Sommerfeld, Theoretischer Physiker. Catalogue of Sommerfeld-exhibition*, Deutsches Museum München, 1985.

tial equation of a special kind for some boundary conditions. Depending on the form of the conductor the results are different kinds of functions, and one has always to develop into series. Fourier worked not only with the Fourier series, but also with higher trigonometrical functions, Bessel functions etc.

This way to tackle problems was a great attraction for William Thomson in England, the later Lord Kelvin, and for Georg Simon Ohm and Franz Neumann in Germany. Ohm published in 1827 his book with a mathematical analysis of the strenght of electrical currents in wires of different lengths. Neumann created at the university of Königsberg the earliest training center for mathematical physics in Germany (15). What was the opinion of the physicists about these achievements? We now want to show precisely, what physicists thought about Fourier and Ohm.

One typical judgement can be found in the important *Physikalisches Wörterbuch* or *Dictionary of Physics*, published from 1825 to 1845 in 20 volumes. Georg Wilhelm Muncke of Heidelberg wrote about the value of mathematics in physics:

> Since the times of Descartes and Newton one had high esteem for mathematics, but it must be realized that mathematics had been used recently in an exaggerated way by a few Frenchmen and until now by some Germans (16).

This opinion is in the lines of the general scepticism against all non-experimental researches. The second example is more intersting: It is a review about Fourier's and Ohm's books written by the Berlin physicist Georg Friedrich Pohl. Fourier's work has its merits, the reviewer explained, but the theory is without any value for our perception of nature. Fourier had stimulated mathematics, but not physics. In respect to the fundamental question "What is heat", Fourier only used the old and wrong concept, that it is a kind of matter without weight. Ohm must be blamed with respect to electricity for the same reason (17).

Today we are inclined to agree with this judgement. It is an important part of theoretical physics to discuss the principles. In 1914 Einstein had defined for theoretical physics two tasks (18):

1) Formulation of principles, which are valid for a whole field of phenomena.

2) Deduction of the phenomena from the principles.

Undoubtedly the first and (more important) task for theoretical physics was neglected at that time. The critical remarks about Ohm's work of 1827 were in vain. Why?

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(15) A. Wangerin, *Franz Neumann und sein Wirken als Forscher und Lehrer*, Braunschweig 1907, 156. Compare also P. L. Butzer, et al., « Zum bevorstehenden 125. Todestag des Mathematikers Johann Peter Gustav Lejeune Dirichlet... » *Sudhoffs Archiv. Zeitschrift für Wissenschaftsgeschichte*, *68*, 1984, 1-20.

(16) Cf. J. S. T. Gehler, *Physikalisches...*, *quot.*

(17) *Jahrbücher für wissenschaftliche Kritik*, Berlin 1828, vol. 1, no. 11/12-13/14, columns 85-86, 97-103. Copy available at Deutsches Museum, Ohm Papers.

(18) A. Einstein, *Mein Weltbild*, new edition, Berlin 1971, 110 f.

With a small group of friends Pohl was an outsider in the scientific community. These outsiders were close to the romantic natural philosophy of Schelling and Hegel. For Schelling and Hegel observation and experiment was the method of fools, speculation the method of geniuses. Schelling and Hegel and all their followers had no problems to explain in one theory all phenomena of the universe. For a physicist it was easy to realize that this already had been done in former times by Descartes, and since Newton everybody knew, that theories of this kind are of no value at all for physics.

Speculation of Schelling's kind caused amongst physicists a real hate against new ideas. In the scientific community a new hypothesis was regarded as a first step into the desert of speculation, in which it is so easy to lose one's way. When Hermann von Helmholtz wanted to publish in 1847 his famous talk " On the Conservation of Force ", the physical establishment was hostile. They feared that these theoretical considerations might stimulate again the phantastic ideas of Hegel's natural philosophy (19).

I summarize for the middle of the 19th century: The first and most important task of theoretical physics, as we see it today, to find new principles, was not recognized. The reason was a general opposition against speculation in all forms. The second task, mathematical deduction, was regarded as not especially important. It was believed that new fields could be discovered only by the experimental method.

1.4 – *The further development (1850–1900)*

The social and scientific prejudices were unfavourable for the further development of theoretical physics. Nevertheless there were always a few scholars, who were able to keep themselves free from these prejudices. We have to mention first Hermann von Helmholtz. He was strictly against speculation à la Schelling and Hegel, but realized that it was indispensable to analyse the basic principles. He worked on electrodynamics. There was an older theory of Wilhelm Weber, based on the concept of " force-at-a-distance ". James Clerk Maxwell had developed a new field theory, based on Faraday's concept of force. Helmholtz established a third theory, which contained both the theory of Weber and of Maxwell. Helmholtz deduced as consequence the possibility of longitudinal waves. When Röntgen discovered in 1895 his X-rays, he erroneously believed, that he had found these longitudinal waves.

Helmholtz tried also to find differences between the two theories, and opened the way for his student Heinrich Hertz. By a mathematical analysis Hertz showed, that Weber's theory is incomplete. If this theory is completed, the result is Maxwell's theory. So Helmholtz and Hertz successfully fulfilled what Einstein later called the first task of theoretical physics. But Helmholtz also

(19) L. Königsberger, *Hermann von Helmholtz*, 3 vols., Braunscheig 1902–1903, vol. 1, 79.

realized the second task. Just to give one example: he deduced from the fundamental laws of hydrodynamics special solutions, the famous vortex motion.

Helmholtz made also important discoveries in the field of experimental physics. He had a full professorship for physiology and took over in 1871 a chair for physics in Berlin. The Prussian government established for him the biggest and best equipped physical institute. For him experimental and theoretical physics were only different methods of the same field (20). To fulfill his wishes, a second chair was established at the university of Berlin for Gustav Kirchhoff, a chair for theoretical physics. Helmholtz' idea was *not* to concentrate now on experimental physics, but simply to be relieved from his teaching burden. He was a " star ", and the government fulfilled generally his wishes.

Later on Max Planck spoke of " four stars ", which illuminated theoretical physics around 1880, and he had in mind Hermann von Helmholtz, Gustav Kirchhoff, Rudolf Clausius and Ludwig Boltzmann (21). All of these occupied full professorships. We will see, how these chairs developed until the turn of the century. Two of the four stars were directors of an institute and therefore responsible for experimental physics as well. In 1888 a successor came for Helmholtz, August Kundt, a convinced experimentalist. For Clausius in Bonn Heinrich Hertz came in 1889, who worked both on experimental and theoretical physics with the greatest success. But Hertz died young and Heinrich Kayser was appointed, who made the institute an experimental center of spectroscopy.

When Ludwig Boltzmann went back to Vienna in 1894, his chair in Munich was cancelled. So altogether three chairs got lost for experimental physics until the end of the century. Only one of the " four stars " got again a theoretical physicist as successor: when Kirchhoff died, Planck was called to Berlin, where he worked from 1889 for more than five decades. In his memoirs Planck gave an instructive description of the attitude of his collegues towards theoretical physics.

> The full professor of physics at the technical higschool, a profound experimentalist, was always very kind to me, but I always had the feeling that he regarded me as superfluous. I was far and wide the only theoretician, so to say a physicist sui generis [...] I realised also, that the assistant professors of the institute of physics [of the university] always showed a certain reservation (22).

Beside the four " stars ", there were around 1880 two other chairholders. In Königsberg old Franz Neumann gave his lectures, in Göttingen Benedict Listing. Until the end of the century both men were replaced again by theoretical physicists. So in 1900 from altogether 35 chairs or full professorships in physics in the whole *Reich*, only three were for theoretical physics. Berlin with Planck, Göttingen with Voigt and Königsberg with Volkmann. In his letter of 1898,

(20) W. Wien, " Helmholtz als Physiker ", *Naturw.*, *9*, 1921, 694-699.

(21) M. Planck, " Theoretische Physik in Deutschland ", in M. Planck, *Physikalische Abhandlungen und Vorträge*, vol. 3, Braunschweig 1958, 209-218.

(22) M. Planck, " Wissenschaftliche Selbstbiographie ", in M. Planck, *Physikalische ..., cit.*, vol. 3, 374-401.

which I quoted in the beginning of my talk, in which Wilhelm Wien regretted that there were only two full professorships, he had forgotten the chair in Königsberg.

All the other associate professors, officially nominated for theoretical physics, did not count very much, for the first reason, that in the academic hierarchy of Imperial Germany an associate professor was just nothing, and for the second reason that these associate professors for theoretical physics were not real theoreticians. They only waited for a position which would enable them to do experimental physics. Since generally the full professors worked on experimental and the associated professor on theoretical physics, very often the personal hierarchy was extended to the fields. Experimental physics hold the first position and theoretical physics the subordinated second.

2. The 20th century

I now come to the 20th century, but I will deal only with the first three decades.

The situation changed rather rapidly in the first years of the 20th century. In 1915 there were already nine chairs (23). The role of theoretical physics and the opinion about this field changed rapidly, when from 1906 onwards, quantum principles and the special theory of relativity gained general interest. As Nernst said in 1910, physics was " in the midst of revolutionary changes in the fundamentals ", and it was obvious, that theoretical physicists — Planck, Einstein, Lorentz — played a decisive role.

Within a few years Einstein gained a high reputation, which was also a repuation for his field. Already early in 1908 Johannes Stark wished to propose Einstein for a professorship in theoretical physics; his first appointment as associate professor was at Zurich university in October 1909. About this time he read a review paper at the Congress of the Gesellschaft Deutscher Naturforscher und Ärzte on the quantum concept. Planck, acting as chearman, raised objections against some of Einstein's ideas, but undoubtedly he treated young Einstein with the greatest respect. About a hundred physicists were sitting in the audience, amongst them Max Born, Max von Laue, Lise Meitner, Arnold Sommerfeld. Many of them recalled later that it was a sort of official recognition of Einstein's leadership in science (24).

In April 1911 Einstein was nominated full professor in Prague. The whole *intelligentsia* of the city came to his inaugurating lecture, and the biggest lecture hall in the university was occupied until the last seat (25). All educated people were extremely interested; they had heard, that Einstein had achieved new

(23) Cf. W. Lorey, *Das Studium...*, *quot.*, 261.
(24) A. Hermann, *The Genesis of Quantum Theory (1899–1913)*, Cambridge Mass. 1971, 68.
(25) G. Kowalewski, *Bestand und Wandel. Meine Lebenserinnerungen...*, München 1950, 237.

insight into space and time and that this new insight contradicted Immanuel Kant.

In 1914 Einstein got the highest position available for a scholar in Imperial Germany. He became full member of the Royal Prussian Academy. Additionally he was nominated director of a new Kaiser-Wilhelm-Institute for Physics. In his inauguration lecture at the academy Einstein spoke, as already mentioned, about the two tasks of the theoretical physicist. The first task is to find the basic principles, and the method is called induction. The second task is to draw all the conclusions from the principles. For this second task, called deduction, the theoretician is well prepared, and here, provided enough pains and brains, he will not fail. Otherwise with the first task, with induction:

> Here no general method exists, which could be studied and applied systematically. The researcher has to listen carefully to nature to find out these general principles, by realizing at larger complexes of phenomena certain general features, which can be formulated clearly (26).

The new theoretical physics includes the old mathematical physics, but goes — with Einstein's first task — far beyond in accomplishing what philosophy and epistemology of earlier times had wanted to achieve. Theoretical physics really developed to the central and basic discipline of all the sciences. "People complain wrongly, that we don't have philosophers today", Adolf von Harnack said: "They are now only in another faculty, and their names are Planck and Einstein" (27).

Immediately after the First World War Einstein's general theory of relativity gained public interest, and at the same time the atomic model of Niels Bohr stimulated hopes that it would be possible to understand the structure of the atom. Especially the younger people felt enthusiasm. The door is open into new worlds, they were convinced, and now it would be possible to understand macrocosm and microcosm.

Many talented young men were attracted by theoretical physics and found their way into this discipline, among them Werner Heisenberg and Wolfgang Pauli. How was it possible, that theoretical physics developed so rapidly in these postwar years? Thomas S. Kuhn had posed this question. In his answer Heisenberg explained that theoretical physics was the great attraction for well-gifted people:

> I'm personally convinced that the number of interested people is probably about the same all the time, in any century. [...] I would say that the very well-gifted people in the fifteenth century went into painting or [...] sculpture. In the eighteenth century they went into music and they became Mozarts and Beethovens [...] Therefore, I would say, the well-gifted people realized that something was going on in physics. Actually this was my situation when I came from school. I had learned that I could do mathematics easily and I had seen some books on physics. When I had read a few books on this modern development, when I had seen the book of Weyl on relativity and the

(26) A. Einstein, "Prinzipien der theoretischen Physik", in A. Einstein, *Mein..., quot.*, 110-113.
(27) A. Sommerfeld, *Zum siebzigsten Geburtstag Albert Einsteins. Deutsche Beiträge*, vol. 3, 1949, 3.

popular lecture of Sommerfeld on atoms, then I felt this is a field where I could try to do something (28).

Really the "well-gifted people" came into the field, and gathered around Sommerfeld in Munich, Bohr in Copenhagen and from 1921 onwards also in Göttingen around Born and Franck. When a real quantum mechanics was established from 1925 to 1927, a great number of problems could be solved simply by deduction from the new principles. This was task number two, defined by Einstein, in which the theoretical physicist could not fail provided enough pains and brains. Heisenberg called the years from 1927 to 1932 the "golden age of atomic physics", and I quote from his recollections: "Everyone who wanted to do research, to collaborate, to pick from the fruits of the garden, could find numerous problems, unsolvable before, which could now be tackled by the new methods" (29).

The field continued to be the great attraction for young people. Despised in the 19th century, theoretical physicists had won the highest prestige. In their recollections many of them described, that they felt to belong to an elite.

I now come to the end of my talk with a few remarks about the years 1933 to 1945. When the Nazis took over power in Germany, prejudices became part of the official idealogy and policy.

From 1920 onwards some experimental physicists had developed a ressentiment: they regarded it a great danger for science, that their physics, experimental physics, had been placed second rank, and that young men were praised as great physicists, who had never taken an apparatus into their hands. In the wave of anti-semitism, which began also immediately after the First World War, the two prejudices combined, the prejudice against the Jews, and against theoretical physics. Philipp Lenard and Johannes Stark developed strange ideas. They explained that there is an Aryan or German method in physics, the experimental method, which is the only real and good method. And they attacked what they thought to be the wrong and Jewish method of formal construction of abstract and bloodless theories. Many physicists laughed about Stark and Lenard, but when the Nazis came into power, everyone realized their close connections to the Governement and the Party. Theoretical physics was heavily attacked in official journals (30). The numbers of students, who enrolled in this field, decreased rapidly.

Luckily the Nazis were not much interested in science, and therefore only the new developments in the arts but not in physics were officially banned as "degenerated". But this is not my topic today. I just wanted to show the ups and downs of a discipline, theoretical physics.

(28) Sources for History of Quantum Physics, Interview no. 10 with Werner Heisenberg. Cf. German translation, A. HERMANN *Die Jahrhundertwissenschaft. Werner Heisenberg und die Physik seiner Zeit*, Stuttgart 1977, 33.

(29) W. HEISENBERG, *Der Teil und das Ganze. Gespräche im Umkreis der Atomphysik*, Munchen 1969, 131.

(30) A. D. BEYERCHEN, *Scientists under Hitler. Politics and the Physics Community in the Third Reich*, New Haven and London 1977; A. HERMANN, *Wie die Wissenschaft ihre Unschuld verlor. Macht und Missbrauch der Forscher*, Stuttgart 1982.

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